COMMENT

Does the IMF cause civil war? A comment

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Abstract The global economic downturn has heightened concerns about intervention by global financial institutions and political stability. One prominently-published article purports to show that signing on to an IMF structural adjustment program (SAP) increases the risk of civil war, Hartzell et al. (International Organization 64:339–56, 2010). The authors claim that IMF SAPs push liberalization, which hurts people badly enough that they foment civil war. We advance the debate by critically examining their theoretical and empirical evidence, particularly questioning their crucial assumptions about the impact of IMF programs on the economic environment in terms of who actually wins and loses from liberalization and who might be in a position to rebel. Using their data, we find that signing on to an IMF program predicts the onset of a civil war negatively if one uses a lower threshold of 25 deaths when defining civil war. These results suggest that the operationalization of the IMF variable as well as the use of large-scale civil war (1,000 deaths and above) simply capture the effect of ongoing conflict rather than the effects of liberalization. After extending the time period under study and making only minor changes to operationalization, we find that at no time does IMF involvement successfully predict the onset of a civil war.

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1 Introduction

Intervention by the International Monetary Fund (IMF) in crisis-ridden economies is likened to "swallowing the bitter pill" because IMF structural adjustment programs (SAPs henceforth) designed to produce macro-economic stability apparently require deeply-unpopular austerity measures coupled with broad neoliberal reforms often referred to as "the Washington consensus" (Dreher and Rupprecht 2007; Easterly 2005; Stiglitz 2002; Vreeland 2003; Woods 2006).[1] A recently published study by Hartzell, Hoddie and Bauer (HHB hereafter) in *International Organization* argues that liberalization because of IMF SAPs is like "sowing the seeds of war" because the IMF pushes economic liberalization, which generates 'losers' who organize violence. HHB's thesis is that these losers from economic liberalization will have low opportunity costs to rebel. The current economic crisis where people have taken to the streets even in Western capitals make the issue of whether or not the IMF dispenses 'bad medicine' ever more prescient. However, without knowing theoretically who these losers from liberalization are and how exactly they foment violence, it is hard to imagine that there would be no losers willing to fight if economic crisis itself continued. It is also hard from their empirical analysis to separate the effects of the immediate crisis from the liberalizing effects of SAPs, if indeed they actually occur when countries sign an agreement. We examine this issue more closely from both a theoretical and empirical angle and find little support to conclude as they do. Utilizing the very same dataset that they use, we demonstrate that the relationship does not exist, which is good news for countries in crisis that need IMF loans for avoiding even greater hardship.

HHB should be commended for improving on existing studies on IMF programs and armed conflict, a topic that deserves far more attention than has been the case hitherto.[2] They take particular care in solving empirical issues related to selection bias. Using their dataset and operationalization, and making only minor changes, we find results at odds with their conclusions. The positive and significant effect they report of IMF participation on the risk of civil war becomes negative and statistically significant when we change the threshold of deaths from 1,000 to 25, which includes also smaller-scale violence. Moreover, when we replace their main variable of interest, signing on to an IMF program, with a variable capturing whether a country has been under an IMF program for at least 5 months in a year, the effect of IMF involvement on the risk of civil war is never statistically significant. We believe that if the mechanism causing conflict is the effect of IMF programs, then a variable measuring just signing on is inadequate. Assuming, therefore, that participation in a program signifies that a country stays in the program for at least 5 months or more

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[1] See New York Times, *The Bitter Pills in the Plan to Rescue Greece* (30th April, 2010). The 'Washington consensus' is a term coined for specific policy package of liberalization, which pushes the opening up of markets, privatization, the removal of price supports, and the balancing of budgets etc. (Williamson 1994).

[2] Abouharb and Cingranelli (2007) present comparable results in a study using a similar design. Their study covers the years 1981–1999, using the same variable for IMF participation as HHB. Contrarily Rowlands and Joseph (2003) find a negative and significant relationship between IMF programs and civil conflict. Their study is limited to 10 years between 1985 and 1995, and the authors find that their results are not robust to alternative specifications, Bussmann et al. (2005) find no effect of IMF program participation on the risk of civil war in a sample of Sub Saharan African countries. Previous studies, thus, are limited and the results highly mixed.

during the year, adequate time for the effects of liberalization to be felt by the populace, the risk of civil war cannot be predicted by IMF involvement. More interestingly, switching their dependent variable, which measures large-scale civil war (1,000 battle deaths and above) to onset of conflicts determined at 25 deaths and above yields a negative effect that is marginally statistically significant. We also extend the data by 11 years (until 2007) and employ several different operationalizations, but we find no evidence to suggest that the IMF increases the risk of civil war. The rest of this article will examine the theoretical and empirical issues addressed by HHB, present our objections, our alternative empirical strategy and data, present results, and conclude.

2 Blaming doctors for death?

HHB's main argument is that liberalization through IMF programs produces winners and losers and lowers the opportunity costs of losers to participate in civil war, while at the same time reducing the government's ability to compensate them. There are, however, several reasons to be skeptical about these conclusions on theoretical and empirical grounds. First, it is not at all clear from the variable used by HHB (signing on to a SAP) that liberalization occurs when countries borrow from the IMF because of the problem of moral hazard. Governments that get bailout loans can renege on the implementation of reforms, prolong decisions about hard political choices for getting economic fundamentals right, which in fact may lead to stalled reforms and further crises (Bird et al. 2004; Collier and Gunning 1999). Indeed, many find that countries receiving IMF loans do not necessarily liberalize after getting the money (Boockmann and Dreher 2003). The assumption, thus, that SAPs lead to liberalization that cause mass dissent and generate losers, who then go on to organize violence, is quite unfounded and not demonstrated by them.

Contrarily, there are several recent papers that show that more liberalized economies avoid civil and ethnic war and generally have less political repression (de Soysa 2011; de Soysa and Fjelde 2010; de Soysa and Vadlamannati 2012; Mousseau and Mousseau 2008; Steinberg and Saideman 2008). Neither are states that are liberalizing at greater rates exposed to higher political dissent and political repression (de Soysa and Vadlamannati 2012). If it is not liberalization that matters, then it might be that the IMF, who is the 'doctor,' is being blamed for death, particularly in instances where patients refuse the medicine. Studies of the compliance rate of IMF SAPs by countries borrowing from the IMF suggest that on average over half the patients do not take their medicine (Vreeland 2006). HHB anticipate this criticism and suggest that the IMF may cause losers regardless, but as we suggested earlier, governments in crisis are already generating large numbers of losers. The counter factual is what would happen in a country that is facing severe crisis and has no access to 'cheap' IMF loans?

Secondly, who loses out from SAPs and whose opportunity costs for engaging in costly conflict are affected are not clear at all, which has a direct bearing on the assumption about liberalization's effect on the risk of civil war. Those who dissent against SAPs cannot be readily identified as representative of popular opinion. For example, militaries might lose from IMF SAPs, as might political elites, but military

officers are much better placed to rebel than are ordinary people. Indeed, Nooruddin and Simmons (2006) found that military spending was reduced in autocracies after having participated in programs from the IMF. Contrarily, most people are hurt by economic crisis because of high prices, inflation, shortage, and general economic insecurity. HHB, however, are silent on how some groups may overcome collective action problems for organizing violence relative to others. Stylized-theory in terms of winners and losers in the process of liberalization, such as the Heckhscher-Ohlin-Samuelson, Ricardo-Viner type models. suggests that workers and farmers in poor countries will gain from market opening and liberalization whereas capital and domestic rent-seeking forces may lose. Export-oriented businesses are likely to gain relative to inward-oriented ones. Scrutiny by the IMF when countries are in a program is also likely to hurt those rent-seeking coalitions that spend heavily on the military, for example. Privatization and increased competition are likely also to give people access to better quality goods and services at better prices. It is highly unclear from theory, however, to be able to say why ordinary people would foment civil war, particularly at high intensities, simply because they harbour economic and political grievances against a liberalizing government.

Ordinary people will have very high collective action problems for organizing and sustaining large levels of violence. Theoretically, HHB simply suggest that people lose jobs and government budgets get cut due to SAPs, which could have been easily modelled directly, but they do not. Identifying who exactly rebels for what reason is no easy task, so we do not fault HHB for this, but inferring from a positive effect of signing on to a SAP with increased risk of civil war as the effect of liberalization is somewhat suspect when liberalization could have been modelled directly. Also, HHB's explanation is based on the assumption that grievances cause conflict. In other words, the so-called 'losers' actually organize costly violence as a public good that brings redress to others. They do not consider how narrowly-based losers from liberalization, such as military leaders, may actually organize violence for private gain rather than bringing justice to the many. Highly distorted economic fundamentals might also readily be the reason why civil war breaks out, particularly at a time when a bad government tries to reform itself. Distorted economies also generate losers, and economic shocks have been found to increase the risk of armed conflict in its own right (Elbadawi and Hegre 2008). Contrarily, if conflict is driven by the underlying issues that have also caused crisis, then leaving the IMF out and not liberalizing cannot be a solution to peace. There is also a danger that governments will use the argument of stability to spend on the military for increasing security, if indeed reforms generate losers willing to organize costly violence.

We also question HHB's empirical analyses on the following two issues primarily: first, if economic liberalization through IMF programs increases the likelihood of civil war by reducing the opportunity costs of losers, we would expect the effect to unfold over some time. A standard IMF program lasts over a certain period of time, and loans are disbursed in tranches based on how satisfied the IMF is with the reforms undertaken or when the borrowers reach particular reform and policy benchmarks set by the IMF (Kahn and Sharma 2006). Thus, we test the proposition that not only the first, but all years a country participates in such programs matter for conflict. We assess, in any given year, to what extent the IMF has been involved in a country. If liberalization, or the content of SAPs are bad for people, then we should observe

rising grievances and the lowered opportunity costs happening over time. For example, the Liberian case, as presented by HHB illustrates this point well because the standby arrangements the authors suggest influenced the onset of war there were signed between 1980 and 1984, while the actual war did not break out until 1998. If their reasoning is right, then this is because Liberia implemented SAPs according to IMF wishes over those years. However, the evidence from the case they discuss suggests otherwise. Liberia was denied all further credit from the IMF and World Bank in 1985 because the government refused to accept the conditions after signing (Claessen and Salin 1991). The Bank and Fund left the country the same year (Claessen and Salin 1991: 136). Did Liberia, then, suffer civil war because the country was liberalizing due to IMF conditions, or because of deepening crisis?

Second, HHB's study is limited to full-scale civil wars covering only armed conflicts with a threshold of 1,000 or more battle related deaths with at least 100 killed on each side every year. This threshold excludes all but a few of the most brutal conflicts and comprises not more than 74 conflicts in all. Indeed, there is no reason to believe that the IMF causes only the most brutal wars. A country could have experienced several years of devastating conflict before it is recorded as a full-scale civil war. Using the 1,000 death threshold therefore excludes many relevant conflicts, and has some additional unfortunate consequences. For instance, Chad has suffered several rebellions, insurgencies and struggles for power among various factions, warlords and rebel groups since 1965. Yet, there is no civil war in Chad according to their definition until 1994 (Fearon and Laitin 2003; Hartzell et al. 2010). Again, looking closer at this case we discover that the central government of Chad signed a program with the IMF with the intention of rebuilding the economy that had been ravaged by roughly three decades of warring (see also Buhaug 2010). In the data we use, the PRIO/Uppsala Armed Conflict Dataset (ACD hereafter) (Gleditsch et al. 2002; Harbom and Wallensteen 2009), which uses the much lower 25 battle-related deaths in a single year, Chad's civil war onset occurs in 1976.

This difference in the battle-death threshold used for coding a civil war is important because civil war destroys infrastructure and economic activity, which has a direct impact on the independent variables such as per capita GDP, economic growth, and external trade. At the same time, the IMF formally included post conflict assistance in the IMF emergency assistance arrangement in 1995 and has been providing technical advice and programs for reconstruction of war economies before that through other of its facilities. The IMF is also active in countries where conflicts are ongoing (Gupta 2005). Mozambique, for instance, signed its first economic rehabilitation program with the IMF in 1987 during the civil war as part of the transformation from the planned economy to a market economy (Michailof et al. 2002). In this case too, HHB's data record this conflict 10 years after relatively large-scale organized violence had started, and 5 years before it officially ended. For all of these reasons, it is important to identify the onset of the conflict at the earliest time possible, since ongoing conflicts and economic crises are likely to be related. To analyze the relationship between IMF programs and civil war, we use the 25 battle-deaths threshold, which allows greater variance and is by now the standard in the field.

3 Data and methods

In the first instance, we replicate HHB's results on the effects of IMF programs on the risk of civil war during the period 1970–1999. Secondly, we extend their dataset from 1999 to 2007 using alternative models and data that are better reflections of the general literature on IMF lending and on the causes of civil war. HHB conduct bivariate probit analysis in order to mitigate problems associated with endogeneity and omitted variables bias because the conditions causing civil war and the conditions that determine why IMF agreements are signed might not be independent of each other. They account for selection effects, or the non-randomness of the chances of being selected into an IMF program, which is an improvement on existing studies on the subject. The factors hypothesized by HHB to predict the outbreak of civil war are per capita GDP (log), the growth rate of GDP, foreign exchange reserves, democracy measured by the Polity index, the democracy measure squared to model any curvi-linear effects of democracy on civil war, total population (log), a discrete variable taking the value 1 for oil exporting countries and 0 for others, a discrete variable taking the value 1 if a country has suffered civil war the previous year and 0 if not, a count of years under an IMF program, the existence of mountainous terrain, and finally a discrete variable taking the value 1 if a country has signed an IMF program in any given year and 0 if not, which is HHB's main variable of interest. In the first step of the bivariate probit analysis, HHB use the variables just discussed as determinants of SAP programs (see appendix 1 and 2 for details on the variables used by HHB). We make no alterations to either their data or method, which makes our analyses directly comparable with theirs.

We make only two changes. First, we replace their main independent variable, signing on to a SAP, with our preferred variable, which indicates whether or not a country has been in an IMF program for more than 5 months in a given year (Dreher 2006). Secondly, we switch the dependent variable from large-scale civil war (with 1,000 deaths and above) to the onset of conflict measured at 25 deaths and above in a single year (Gleditsch et al. 2002; Harbom and Wallensteen 2009).

In the second part of the analysis, we extend the period of study from 1999 to 2007 covering 192 countries and employ several different variables and data. The variables and data we employ are standard in the literature on why the IMF lends and the literature on civil war. The question of the validity of instruments to control for endogeneity is particularly thorny. In order to control for endogeneity, we need an instrument that is correlated with a country's participation in an IMF program but has no effect on the onset of civil war independently. HHB use the number of countries under IMF programs as the instrument. HHB argue that the variable measuring "the total number of other countries in the world currently under IMF agreement is one that has substantial predictive value for IMF loan participation and is exogenous with respect to civil war onset" (p. 348). We are not fully convinced. If as they suggest participation in an IMF program causes civil war, then neighboring countries already under IMF programs might be affected by contagion effects from the neighborhood rather than by IMF effects independently.[3] HHB do not provide any tests, which

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[3] See Bosker and De Ree (2011) for numerous examples of civil war spillover in different parts of the globe.

satisfy the instrument relevance and exclusion restriction criteria. Several studies find a strong relationship between voting pattern in the United Nations General Assembly (UNGA henceforth) and IMF lending (see Dreher and Sturm 2012; Barro and Lee 2005; Stone 2004; Thacker 1999), which we prefer as an instrument.

We employ Kegley and Hook's measure of voting in line with the US as our instrument since it is often argued that the US, which has the most votes, rewards its allies with IMF support (Kegley and Hook 1991; Stone 2004). The Kegley and Hook (1991) measure codes votes in agreement with the US as 1, votes in disagreement as 0, and excludes the abstentions.[4] The resulting numbers are then divided by the total number of votes in the UNGA each year. The data are sourced from Dreher and Sturm (2012).

Moreover, we refine the models and data used by HHB in our extended analyses. For example, they use the Polity index to measure democracy. Polity, however, is sensitive to ongoing violence (Cheibub et al. 2010). They also square the democracy measure to model the quadratic effect of democracy on civil war, which is not generally recommended given problems with measurement bias associated with such measures (Treier and Jackman 2008). Moreover, the results of the squared term in nonlinear estimations, such as the bivariate probit technique, are not easy to interpret. We use a variable measuring the degree of democracy that is less prone to bias from ongoing conflict developed by Cheibub et al. (2010) which distinguishes regimes according to whether executive and legislative offices are filled through contested election.[5]

In the civil war equation (second stage), the dependent variable is civil war onset with 25 deaths in a single year taken from ACD. Our main variable of interest remains a discrete variable flagging whether or not a country has been in an IMF program for more than 5 months during the year. We include the standard controls viz., per capita GDP (log), population (log), trade openness measured as trade to GDP, the degree of democracy, a count of the years of peace since the last conflict, a discrete variable flagging oil exporting countries, and the existence of mountainous terrain. Following Balla and Reinhardt (2008), we also add a discrete variable taking the value 1 if there is conflict in the neighborhood and 0 if not to control for spatial dependence and regional geopolitical factors that may simultaneously influence IMF involvement and cause conflict.

In the IMF equation (first stage), where the dependent variable is a discrete variable flagging if a country has been in an IMF program for more than 5 months during the year or not, we include economic indicators that influence the decision to apply for IMF assistance (Barro and Lee 2005; Bird and Rowlands 2006; Dreher 2004). Following others, including HHB, we use per capita GDP (log) and rate of growth of GDP. We include the availability of foreign reserves measured as the total amount of reserves as months of exports of a country, which also serves as a proxy for the extent of a country's vulnerability to external shocks (Barro and Lee 2005). Trade to GDP is used for the degree of trade openness because closed economies are known

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[4] As a further robustness check, we replace Kegley and Hook's (1991) measure of UNGA voting with Thacker's (1999). He codes votes in agreement with the US as 1, votes in disagreement as 0, and abstentions as 0.5. Our results remain robust to using UNGA voting based on Thacker's (1999) index.
[5] See Cheibub et al. (2010) for a detailed discussion on classifying democracies and dictatorships.

to go to the IMF more often (Bird and Rowlands 2006). We also include three dummy variables indicating whether a country has experienced a currency crisis, a debt crisis, and a systemic banking crisis (Laeven and Valencia 2008). Finally, we include the number of consecutive years a country has been under an IMF program during the time period under study (Abouharb and Cingranelli 2007). The definitions and sources of all variables are provided in appendix 2.

4 Results

Table 1 reports the replication results. In column 1, we replicate the positive and statistically significant effect reported by HHB for their variable measuring signing on to an IMF agreement on civil war. The coefficient we obtain of 1.885 is extremely close to that reported by HHB (1.91). In fact, the difference is due to 1 missing observation resulting from merging our data with theirs. Holding all other variables constant, in column 2, we replace the variable signing on to an IMF agreement with our discrete variable measuring whether a country has been under an IMF agreement for more than 5 months in a year. This variable is statistically not significant. In other words, staying under an IMF program for at least over 5 months in a financial year has no significant effect on the outbreak of civil war in the same model where signing on to a SAP is statistically significant. Given that these tests were based on the 1,000 battle deaths threshold capturing severe civil war, which should take much greater organization and time to take effect, it is hard to accept that signing on to an IMF matters and that being in an IMF program for over 5 months in a year does not.

In column 3, we replace the 1,000 battle-death criteria for a civil war outbreak with the 25 deaths threshold. Surprisingly, HHB's variable measuring signing on to an IMF agreement is now negative and marginally significant at the 10% level. These results suggest that HHB's measure signalling signing on to an IMF SAP is not robust to alternative measurement of the risk of civil war. In column 4, however, our preferred measure of the five-months duration in an IMF program within a year shows no predictive power in terms of an onset of civil war also for the lower battle-death threshold.

In Table 2, we examine our updated data, including various forms of crises that elicit IMF interventions and the UNGA voting index as our instrument in the first stage. In column 1, we use HHB's measure of IMF signed while in column 2, we replace it with our measure of whether a country has been under an IMF program for at least 5 months. As seen in column 1 and 2, none of the IMF variables used (HHB's and ours) significantly predict the onset of a civil war (Table 2).[6] In fact, of 200 recorded economic crises in our dataset, only 16 occur in the same year as a civil war onset (>25 deaths). Furthermore, in a total of 97 events where a country both experienced an economic crisis and signed an IMF program, only five occurred in the same year as a civil war.[7] In other words, only 5 of 16 total crises associated with civil war had IMF involvement. Notice that the UNGA variable is positive and

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[6] Note that the results of signing on to the IMF also remain statistically insignificant when including the count of years a country has been under an IMF program.

[7] The results are presented in Table 2 of the online appendix available on this journal's website.

Table 1 Bivariate Probit Estimations Replicating Hartzell et al. (2010)

	Fearon & Laitin>1000 (1)	Fearon & Laitin>1000 (2)	ACD >25 battle deaths (3)	ACD >25 battle deaths (4)
Civil war equation				
Signed IMF program dummy	1.885***		−1.022*	
	(0.592)		(0.525)	
IMF participation>5 months dummy		0.282		−0.0921
		(0.711)		(0.523)
GDP per-capita (log)	−0.0690	−0.0766	−0.106***	−0.0776***
	(0.0428)	(0.0581)	(0.0328)	(0.0301)
GDP growth rate (t-1)	−0.00159	−0.00473	0.00436	0.00955
	(0.00698)	(0.00804)	(0.00703)	(0.00710)
Foreign exchange reserves	−0.0266	−0.0297	−0.0271	−0.00496
	(0.0284)	(0.0342)	(0.0235)	(0.0235)
Democracy (polity)	0.00428	0.00330	0.0296***	0.0303***
	(0.0137)	(0.0160)	(0.0102)	(0.0109)
Democracy (polity) squared	−0.00676***	−0.00661***	−0.00348*	−0.00434**
	(0.00208)	(0.00230)	(0.00205)	(0.00220)
Total population (log) (t-1)	0.0526	0.0518	0.129**	0.154***
	(0.0401)	(0.0484)	(0.0528)	(0.0568)
Oil exporting countries dummy	0.397*	0.482**	0.331*	0.375**
	(0.225)	(0.223)	(0.170)	(0.180)
Previous civil war dummy	−0.0897	−0.147	0.00469	0.0180
	(0.112)	(0.119)	(0.111)	(0.129)
Count of years in IMF program (t-1)	0.00615	0.00757	0.00634	0.00341
	(0.00788)	(0.0128)	(0.00715)	(0.0113)
Mountainous terrain	0.101**	0.114**	0.00687	0.00546
	(0.0479)	(0.0538)	(0.0409)	(0.0483)
Constant	−2.239***	−2.275***	−2.236***	−2.925***
	(0.387)	(0.522)	(0.798)	(0.598)
IMF program				
GDP per-capita (log)	−0.133***	−0.194***	−0.133***	−0.194***
	(0.0184)	(0.0274)	(0.0181)	(0.0274)
GDP growth rate (t-1)	−0.0106*	−0.00749	−0.0107*	−0.00755
	(0.00554)	(0.00501)	(0.00582)	(0.00500)
Foreign exchange reserves	−0.0732***	−0.0627**	−0.0730***	−0.0625**
	(0.0239)	(0.0253)	(0.0243)	(0.0252)
Democracy (polity)	0.0147*	0.00258	0.0140*	0.00245
	(0.00756)	(0.00938)	(0.00759)	(0.00931)
Democracy (polity) squared	−0.000156	−0.00242	−0.000105	−0.00240
	(0.00166)	(0.00223)	(0.00171)	(0.00222)
Total population (log) (t-1)	−0.00602	−0.0405	−0.00534	−0.0405
	(0.0296)	(0.0401)	(0.0287)	(0.0402)

Table 1 (continued)

	Fearon & Laitin>1000	Fearon & Laitin>1000	ACD >25 battle deaths	ACD >25 battle deaths
	(1)	(2)	(3)	(4)
Oil exporting countries dummy	0.0325	0.00281	0.0549	0.00481
	(0.102)	(0.178)	(0.0994)	(0.176)
Previous civil war dummy	0.0281	−0.120	0.00840	−0.120
	(0.0932)	(0.170)	(0.0926)	(0.169)
Count of years in IMF program	0.0150**	0.0334***	0.0160**	0.0333***
	(0.00632)	(0.00739)	(0.00632)	(0.00742)
Number of countries in IMF program (t-1)	−0.00698**	0.0148***	−0.00758**	0.0149***
	(0.00327)	(0.00462)	(0.00328)	(0.00463)
Constant	−0.120	−0.187	−0.108	−0.192
	(0.307)	(0.451)	(0.306)	(0.446)
Number of observations	2404	2404	2404	2404
Log likelihood ratio	−1103.68	−1275.50	−1255.84	−1426.96

Robust standard errors in parentheses; Independent variables in civil war equation lagged following Hartzell et al. (2010) in column 1 and 3; * $p<0.10$, ** $p<0.05$, *** $p<0.01$

significantly different from zero at the 1% level, suggesting that countries entering into IMF programs are significantly more likely to vote with the US in the UN General Assembly. It is also noteworthy that countries experiencing debt and financial crises are more likely to enter into an IMF program. Also, our results from civil war models are identical with the findings of others, particularly Fearon and Laitin's (2003) findings where per capita income, population size, and oil exporters display the expected signs and are statistically significant at conventional levels (Hegre and Sambanis 2006; Ward et al. 2010). Given that IMF interventions are likelier where crises are present, as shown in our results, it is likely also that IMF interventions possibly assist the maintenance of peace, since without access to IMF finance poor countries in crisis may end up falling deeper into trouble and suffer austerity by default, leading ultimately to state failure.

4.1 Further checks on robustness

We examine the robustness of our findings from the extended dataset in several ways. First, we use an alternative measure of civil war outbreak, wherein we use the incidence of civil war, a variable coded 1 for each year if a country is in conflict and 0 otherwise. Although it is equivalent to Fearon and Laitin's civil war measure, in this version it records every year that the death toll surpasses 1,000 deaths. Replicating the extended data's models with this alternative measure of civil war shows no association between participation in an IMF program and outbreak of civil war. Second, we drop OECD countries from our extended dataset and re-estimate the specification shown in Table 2. Again, we find no statistically significant effects of IMF program participation on civil war.

Table 2 IMF Intervention and the onset of civil war using extended data, 1970-2007

	ACD >25 battle deaths (5)	ACD >25 battle deaths (6)
Civil war equation		
Signed IMF program dummy	0.0243	
	(0.510)	
IMF participation>5 months dummy		0.478
		(0.396)
Total population (log)	0.187***	0.191***
	(0.0425)	(0.0422)
GDP per-capita (log)	−0.200***	−0.128
	(0.0692)	(0.0840)
Trade openness	0.00190	0.00217
	(0.00181)	(0.00178)
Democracy (Cheibub et al.)	0.155	0.119
	(0.119)	(0.120)
Peace years count	−0.00984*	−0.0109*
	(0.00570)	(0.00559)
Oil Exporting countries dummy	0.399***	0.425***
	(0.150)	(0.145)
Neighbor at war dummy	0.00489	−0.00505
	(0.136)	(0.133)
Mountainous terrain	−9.33e-05	−0.000232
	(0.00290)	(0.00286)
Constant	−7.944***	−8.818
	(1.581)	(39.49)
IMF program		
GDP per-capita (log)	−0.259***	−0.572***
	(0.0301)	(0.0304)
GDP growth rate	−0.0137***	−0.00369
	(0.00464)	(0.00385)
Trade openness	0.000143	0.000678
	(0.001000)	(0.000887)
Foreign exchange reserves	−0.0666***	−0.0351***
	(0.0145)	(0.0116)
Currency crisis dummy	0.356**	0.177
	(0.139)	(0.145)
Debt crisis dummy	0.402*	1.208***
	(0.223)	(0.260)
Banking crisis dummy	0.261	0.230
	(0.160)	(0.156)
Ongoing civil war dummy	−0.188**	−0.368***
	(0.0873)	(0.0774)

Table 2 (continued)

	ACD >25 battle deaths (5)	ACD >25 battle deaths (6)
UNGA voting alignment index	1.954***	3.509***
	(0.520)	(0.451)
Count of years in IMF program	0.0309***	0.0487***
	(0.00390)	(0.00350)
Constant	0.0178	1.258**
	(0.580)	(0.612)
Number of observations	2853	2853
Log likelihood ratio	−1377.05	−1628.66

Robust standard errors in parentheses; Time dummies not shown; * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Third, we control for country fixed effects along with the time fixed effects (which are already controlled for) using two-stage least squares method (2SLS-IV hereafter), or linear probability models. In doing so, we use voting in line with the US in the UN General Assembly as our instrumental variable. Note that we estimate 2SLS-IV models by including time dummies in one specification and including time and country dummies in another specification. The validity of the selected instruments depends on instrument relevance, i.e., the instrument must be correlated with the explanatory variable in question. Bound, Jaeger and Baker (1995) suggest examining the F-statistic on the excluded instruments in the first-stage regression. The selected instruments would be relevant when the first stage regression model's F-statistics meet the thumb rule threshold of being above 10 (Staiger and Stock 1997). Secondly, the instrument variable should not vary systematically with the disturbance term in the second stage equation, i.e., $[\omega_{it}|IV_{it}]=0$ meaning, the instruments cannot have independent effects on the dependent variable. The joint F-statistic in our models is always above 10, which is significantly different from zero at the 1% level.[8] With only one instrumental variable it is rather difficult to test the overidentifying restriction. However, we regress civil war onset on our instrumental variable holding all other control variables constant.[9] After controlling for endogeneity associated with IMF program participation, and for country fixed effects, we do not find any difference in relation to the effects of IMF program participation. The impact of IMF program participation on the onset of civil war remains statistically insignificant. Our extended data show that IMF involvement is a poor predictor of civil war, a result supported by others who have used different data and methods examining this question for African countries (see Bussmann et al. 2005).

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[8] The results appear in Table 3 in the online appendix on this journal's website. Apart from joint F-statistics, we also use the Cragg-Donald test of the null that the model is under identified, i.e., the UNGA voting index does not sufficiently identify IMF program participation (Cragg and Donald 1993). However, the Cragg-Donald test allows us to reject the null hypothesis that the model is under identified.

[9] We find no statistical significance of the UNGA voting index constructed using Kegley and Hook's method on the onset of civil war. However, the UNGA voting index constructed using Thacker's method did not pass the test of overidentifying restriction. These results are also displayed in an online appendix (Table 4) on the journal's website.

5 Conclusion

To date, few studies have looked at the impact of IMF lending on the risk of civil war. Recently, Hartzell et al. (2010) purport to demonstrate that IMF lending causes civil war by liberalizing economies, which in turn, leads to losers who foment civil war. Indeed, their study, which takes the issue of non-randomness between IMF programs and civil war seriously is an improvement on previous empirical studies on the subject. Using this study as a benchmark, we investigate the issue further by critically examining some fundamental assumptions in their study, namely that the IMF actually liberalizes countries and that liberalization is what causes the conditions of conflict, rather than crisis. Unfortunately, their explanation of the connection is neither grounded well in theories explaining who wins and who loses from liberalization, nor who is in a position to engage in costly rebellion when IMF programs take effect.

Given their explanation of why IMF programs cause violence, the use of an empirical strategy that employs a contemporaneous term for signing on to an IMF program is somewhat unconvincing. With only minor adjustment to their own data, particularly the use of a dependent variable measuring the onset of a civil war at the lower threshold of deaths, we find ample evidence suggesting that the relationship they report is not stable and may in fact be the opposite. Our extended data and a better operationalization of a model of civil war, plus controlling for endogeneity between conditions of crisis and IMF involvement, show no connection—either positive or negative—between participation in IMF programs and the risk of civil war. Further research is clearly needed on the subject, particularly the use of alternative operationalizations of IMF involvement and data measuring the degree to which governments liberalize. Indeed, two variables, per capita income and population size, seem to explain most of the variance in civil war onsets (Ward et al. 2010). Future research might focus on the rent-seeking bases on which conflict is fomented when poor countries attempt reforms for securing their economic futures.

Appendix 1

Table 3 Descriptive statistics

HHB data	Mean	Standard deviation	Minimum	Maximum	Observations
Fearon Laitin's onset>1000 battle deaths	0.017	0.130	0.000	1.000	4238
Signed IMF program	0.141	0.348	0.000	1.000	4226
GDP per capita (log)	4.259	4.730	0.196	41.021	4094
GDP growth rate (t-1)	1.634	6.331	−41.900	63.630	3303
Foreign exchange reserves (t-1)	3.479	2.988	0.000	25.000	2760

Table 3 (continued)

HHB data	Mean	Standard deviation	Minimum	Maximum	Observations
Democracy: polity2 index	−0.329	7.572	−10.000	10.000	4219
Democracy squared: polity2 index	57.435	30.890	0.000	100.000	4219
Total population (log) (t-1)	9.079	1.481	5.403	14.030	4238
Oil exporting countries dummy	0.151	0.358	0.000	1.000	4238
Previous civil war dummy	0.146	0.353	0.000	1.000	4238
Number of countries in IMF program (t-1)	6.037	7.297	0.000	35.000	4225
Mountainous terrain	2.090	1.431	0.000	4.557	4238
Count of years in IMF program	44.550	16.016	21.000	72.000	4078
IMF participation >5 months dummy	0.197	0.398	0.000	1.000	5760
Democracy dummy (Cheibub et al.)	0.409	0.492	0.000	1.000	4832
Extended analysis					
ACD onset >25 battle deaths	0.033	0.179	0.000	1.000	6013
Signed IMF program	0.127	0.333	0.000	1.000	6013
IMF participation >5 months dummy	0.255	0.436	0.000	1.000	6013
Total population (log) (t-1)	15.763	1.707	11.620	20.999	5874
GDP per capita (log)	7.389	1.588	4.046	11.686	5521
Trade openness	74.557	43.266	0.309	438.091	5372
Democracy dummy (Cheibub et al.)	0.429	0.495	0.000	1.000	5985
Peace years count	11.598	10.978	0.000	38.000	6013
Oil exporting countries dummy	0.147	0.354	0.000	1.000	5974
Neighbor at war dummy	0.156	0.363	0.000	1.000	6013
Mountainous terrain	17.347	21.659	0.000	94.300	5470
Count of consecutive years in an IMF program	7.768	8.778	0.000	42.000	5869
GDP growth rate (t-1)	4.016	10.049	−88.086	446.865	4311
Foreign exchange reserves (t-1)	3.634	3.293	−0.092	43.694	4384
Currency crisis dummy	0.035	0.183	0.000	1.000	5767
Debt crisis dummy	0.011	0.102	0.000	1.000	5767
Systemic banking crisis dummy	0.021	0.144	0.000	1.000	5767
Ongoing civil war incidence	0.170	0.375	0.000	1.000	6013
UNGA voting alignment index	0.194	0.116	0.000	0.734	5581
Fearon Laitin's onset>1000 battle deaths	0.017	0.131	0.000	1.000	4086

Appendix 2

Table 4 Data definitions and sources

Variables	Definition	Source
ACD onset >25 battle deaths	Onset of intrastate conflict with more than 25 battle deaths. Dummy coded 1 if there is more than two years since the previous onset	UCPD/PRIO Armed Conflict Dataset Version 4–2012, Gleditsch et al. (2002); Themnér and Wallensteen (2012)
Ongoing civil war incidence	Dummy coded 1 for each year a country has at least one active conflict.	UCPD/PRIO Armed Conflict Dataset Version 4–2012 Gleditsch et al. (2002); Themnér and Wallensteen (2012)
Signed IMF program	Dummy takes the value 1 if a country has signed an IMF program in a year and 0 otherwise	Dreher (2006)
IMF participation >5 months dummy	Dummy takes the value 1 if a country has been in an IMF program for more than five months during the year and 0 otherwise	Dreher (2006)
Count of years in IMF program	Count of years if a country has been or is under an IMF program	Abouharb and Cingranelli (No date), Vreeland (2003)
GDP per capita (log)	GDP per capita	World Development Indicators (World Bank 2011)
GDP growth rate (t-1)	Rate of growth of GDP of a country	World Development Indicators (World Bank 2011)
Trade openness	Exports and imports as % of GDP	World Development Indicators (World Bank 2011)
Oil exporting countries dummy	Dummy takes the value 1 if a country's oil exports are 1/3rd of GDP	Authors' own coding
Foreign exchange reserves	Foreign reserves as the number of months of export of a country	World Development Indicators (World Bank 2011)
Currency crisis dummy	Dummy takes the value 1 if a country is exposed to currency crisis	Laeven and Valencia (2008)
Debt crisis dummy	Dummy takes the value 1 if a country is exposed to debt crisis	Laeven and Valencia (2008)
Systemic banking crisis dummy	Dummy takes the value 1 if a country is exposed to banking crisis	Laeven and Valencia (2008)
Total population (log)	Total population in a country logged.	World Development Indicators (World Bank 2011)
Mountainous terrain	Proportion of country being mountainous	Fearon and Laitin (2003)
Democracy dummy	Dummy takes the value 1 if a country is classified as a democracy	Cheibub et al. (2010)
Peace years count	Count of civil peace years between two civil war onsets	Authors own coding

Table 4 (continued)

Variables	Definition	Source
Neighbour at war dummy	Dummy takes the value 1 if a neighbouring country is engulfed in civil war and 0 otherwise.	Authors' own coding
UNGA voting alignment index	The index is constructed based on the Kegley and Hook (1991) which codes votes in agreement with the US as 1, votes in disagreement as 0, and excludes the abstentions. The resulting numbers are then divided by the total number of votes in UNGA each year resulting in an index between 0 and 1.	Dreher and Sturm (2012)

All variables in Table 1 are replication data from Hartzell et al. (2010), except ACD onset >25 deaths and IMF participation >5 months dummy, which are as described in this table

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